On the Interior Satellites of Uranus. By Mr. Lassell.

Extract of a letter dated Nov. 13, 1851:—

"After a ten-day painful exercise of patience, I obtained last night, still under great disadvantages, another view of the two new satellites of *Uranus*. The sky was so generally cloudy and always hazy through the whole of last night, that I could get no absolute measures (save one) of either position or distance of either even of the bright satellites; though I managed to get a view of both the close ones, and also as good estimations of their situations with respect to the bright ones, as convinced me that no great error of position could exist. In default of measures, I estimated the position of IV to be 5°, and of III to be 110°. I afterwards obtained a single rough measure of III which gave, position 112° 34′; 2 was estimated, in distance *fully* six-tenths of the distance of III; 1 was decidedly closer, and barely one-third of the distance of IV: 2 obviously brighter than 1, as it also was on the 2d November. I made a diagram of the positions of the four satellites in my observation book with as much care as possible; and I inclose you a copy of it reduced to a scale of 1″ = 0·05 inch, the angles of position being deduced by the application of a protractor. I ought to add that, in order to avoid the possibility of prejudice, I entered into no calculation whatever of the expected positions of the satellites, until all was written down in my book, and I had come in from the observatory. I had, indeed, at the time of observation, no knowledge which was the most likely quadrant of location of any of them. The powers used were 614 and 760, and the epoch $10^h\ 30^m$."

Mr. Lassell finds that a periodic time of 2·506 days will satisfy the place of 1, and a period of 4·150 days the place of 2, and that the estimated elongations agree pretty fairly with the elongations calculated from the periodic times.

"It may, however, be necessary to say a word in reference to M. Otto Struve's, and indeed my own former observations of an interior satellite or satellites (see *Monthly Notices*, 14th January, 1848, and 10th March, 1848). I confess I never thought my own observations sufficiently connected and definite to be worthy of the very ingenious and elaborate discussion given by my friend Mr. Dawes as the basis (in conjunction with M. Struve's observations) of an assumption of periods for three interior satellites. And, indeed, in the memoirs of both these distinguished astronomers, various suppositions are necessarily introduced to account for what would otherwise be irreconcileable observations.

"I cannot, indeed, assert that *Uranus* has no interior satellites besides these two; but I feel very confident, that if they exist, they must be very much fainter than either 2 or 1 (which I now observe easily in good circumstances) or they could not have escaped my notice. It appears to me that the most probable hypothesis to explain M. Otto Struve's and my own former observations, is to conclude that we sometimes observed one satellite and sometimes the

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other; especially as 1 is generally brighter than 2, and if near its greater elongation might be seen when 2 otherwise situated would be invisible. This would explain the difficulty of reconciling the observations."

Extract of a letter dated Nov. 29, 1851:—

"It is rather remarkable that, since I first saw these satellites, I have never looked for them without seeing them, though on some occasions, *e.g.* Nov. 21, the 2d, always the faintest, was recognised with most extreme difficulty. I inclose a tracing of the diagrams made on four more evenings, viz., up to Nov. 22, which, compared with the observations of the 2d, give periods of 2·509 days = $2^d\ 12^h\ 13^m$, and 4·126 days = $4^d\ 3^h\ 1^m$ respectively. I have not yet been able to measure their distances directly and absolutely from the centre of the planet, as its brightness, when fully in the field, always either entirely extinguishes them or renders it impossible to apply the micrometer. I shall, however, if I am not soon favoured with weather sufficiently fine for this purpose, measure the faint ones from the bright ones, and in this way get at their distances. Hitherto all the fine sky I have had has been used in measuring the positions and distances of the old satellites, of which I am getting as many measures as I can during this apparition, with as much accuracy as possible; and when I have finished the series I will send them to the Society. I have also not quite neglected *Neptune;* but I wait also to complete his series. While observing *Uranus* I shall also carefully scrutinise all his neighbourhood, at least within 4′. I may mention that on the 2d inst., the *motion* of 2 in overtaking IV was evident during observation. When first seen it was sensibly short of the line joining the planet's centre and IV, and when last seen it was just perceptibly beyond it. In the various quadrants in which these satellites have been seen, I observe no difference of brightness which cannot be accounted for by their greater or less distance from the planet. I have certainly seen the nearest when within 10″ or 11″, and, therefore, have not found Sir W. Herschel's remark hold (in my experience), that the satellites generally disappeared when they came within about 22″ of the planet. I suppose the greatest elongations of 1 and 2 are now somewhere about 13″ and 18″, and therefore they never can come near Sir William's limits of visibility. I am led to presume that the flare round the planet in his telescope must have been the cause of the satellite not being seen within 22″. In my own telescope I have found differences in this kind of scrutiny which I can only attribute to the state of the air. Sometimes the satellites are more easily measured, and are better seen with the full aperture than with any diminution; and at others, 20 inches shows the close satellites more obviously. I attribute the sudden detection, and frequently repeated observation of these minute and close objects, to the superior action of my mirror under its recently applied system of lever support; it now defines better and brings out with more vividness and concentration minute points of light. The steadiness with which *Mimas* is seen, when

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near its greatest elongation, with a power of 400 or 500, notwithstanding the splendour of *Saturn* and its widely-opened ring full in the field, is a proof of this."

*Table of Mr. Lassell's Observations of the Interior Satellites of Uranus.**

1851.	h m		Position. ° ′	Distance. ″
Nov. 12, at	10 30	IV	5 est.	
		III	112 30	
		2	131	
		1	332	
Nov. 17	10 30	IV	220 12	34·63
		III	257 34	24·09
		2	47	13
		1	337	12
Nov. 18	12 0	IV	191 6	36·83
		III	202 est.	
		2	316	13
		1	186	12
Nov. 21	11 44	IV	131 57	34·29
		III	91 7	21·38
		2	63	10
		1	123	12
Nov. 22	11 36	IV	101 32	
		III	38 50	
		2	329	17
		1	332	12

" Pray mention in the next *Monthly Notice* that in the diagram of the satellites of *Uranus*, vol. xi. p. 247, the words *north* and *south* should *change* places. The additional motion applied to my polishing machine answers well, but requires more attention than I think any body else would give. I wait to make some further trials before I announce it. I have looked twice for D'Arrest's comet with the aid of Mr. Pogson's ephemeris: the circumstances were not very favourable, and I failed to detect it."

D'ARREST'S COMET.

Elliptic Elements. By M. Yvon Villarceau.

" I have employed five observations made at Leipsic and Berlin between June 29 and July 6, and two observations made at Paris July 27 and August 3. The latter is rather doubtful, and I have only employed it from the impossibility of getting others without too great delay.

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* The position of IV on Nov. 12 is *estimated*, as is the position of III on Nov. 18; the other positions of IV and III and the distances have been measured. The positions of 2 and 1 are given by Mr. Lassell from their configurations with the planet and brighter satellites; the distances are taken from Mr. Lassell's diagram according to his scale.

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"A first trial showed me that a satisfactory result could not be deduced by using a method applicable to three observations, which is owing to the very small geocentric latitudes of the comet. The method which I have given for planets in the Additions to the *Connaissance*, 1852, is perfectly suited to this occasion.

Perihelion Passage, 1851, July 8·97942, Paris M.T.

	° ′ ″	
Long. Perihel.	324 10 26·7	Mean Eq.
Ascending Node	149 21 52·2	July 9.
Inclination	14 14 40·7	
Angle (sine = excentricity)	44 43 3·1	
Mean Daily Heliocentric Motion	443·7415	

"Hence we have,

Perihelion Distance	1·185163, log = 0·0737781
Excentricity	0·7036121
Semi-axis Major	3·99869, log = 0·6019177
Sidereal Revolution	7·99608 years.

"The elements represent the observations pretty fairly, though they are liable to considerable modification from future observations. It is scarcely probable that the *periodic* nature of the comet should be changed.

"I, too, had made the remark, which was suggested to the Academy of Sciences by M. Benjamin Waltz, that this comet might probably be the same as the comet of 1678 calculated by Douwes. But I think the perturbations during the interval must be computed before we can pronounce this identity to be proved."

Mr. Norman Pogson, who was lately engaged at Mr. Bishop's Observatory, and is now attached to the Radcliffe Observatory, had the kindness to compute a daily ephemeris of D'Arrest's comet, from the last corrected elements by Dr. D'Arrest, for the interval between Nov. 21 and Dec. 6, 1851. This ephemeris was forwarded to the possessors of powerful telescopes, Mr. Lassell, Professor Challis, &c., in the hope of obtaining some additional observations. Unluckily, from the state of the weather or the faintness of the comet, Mr. Pogson's zeal has not been rewarded by the rediscovery of the comet.

BRORSEN'S FOURTH COMET (1851, *August* 1).

HAVERHILL. (Mr. W. W. Boreham.)

1851.	Green. M.T. h m s	R.A. h m s		N.P.D. ° ′ ″		No. of Obs.	Star.
Aug. 13	9 47 46	14 17 15·03	+0·049 *p*	53 1 42·5	−0·53 *p*	3	*a*
14	11 1 3	19 27·92	0·050 *p*	52 32 15·8	0·65 *p*	3	*b*
18	10 10 47	28 26·60	0·053 *p*	50 37 57·5	0·56 *p*	6	*c*
21	9 38 5	35 43·69	0·052 *p*	49 10 45·5	0·47 *p*	3	*d*
28	9 9 29	14 55 10·91	+0·054 *p*	45 30 39·2	−0·42 *p*	3	*e*

p = Horizontal equatoreal parallax. The places are corrected for refraction.

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Mean Places of Stars of Comparison.

	Name.	R.A. h m s	N.P.D. ° ′ ″
a =	B.A.C. 4797	14 22 5·68	53 7 59·9
b =	— 4778	17 18·78	52 7 2·1
c =	— 4812	26 4·84	51 2 17
d =	Radcliffe Obs. vol. ix. No. 786	34 48·27	49 28 3·7
e =	Lalande, No. 27447	14 56 32·22	45 42 20·5

Occultations of Stars by the Moon, observed at Ashurst, near Dorking. By Mr. Snow.

(Lat. 51° 15′ 58″ North; Long. 0^{h} 1^{m} 10^{s} West.)

	Star.	Ashurst Sid. T. of Immersion. h m s	Limb.	Ashurst Sid. T. of Emersion. h m s	Limb.	
1850. Nov. 8	33 Capricor.	20 17 3·15	dark			
1851. Feb. 21	η Libræ	……	…	16 32 8·7	dark	good.
Mar. 13	d^1 Cancri	8 44 15	dark	9 32 53	bright	good.
	θ Cancri	12 32 29·3	dark			
Sept. 14	845 B.A.C.	20 51 18	bright	22 49 2	dark	good.

Occultations of Stars by the Moon observed by Capt. Shadwell, R.N. F.R.A.S., at the Dockyard (Flag-Staff), Trincomalee.

Latitude of the Flag Staff, 8° 31′ 26″ North.

1851.		Mag.		Local M.T. h m s
July 12	ο Sagittarii	4.5	Immersion	9 1 34·2
15	ζ Capricorni	3.4	Emersion	15 39 41·1

A further memoir has been received from Mr. Maclear containing results of his comparison of the southern stars of the B.A.C. Catalogue with the heavens. The *observed* places depend upon from two to four observations with the 10-foot transit, and as many with the mural circle.

In assigning the probable source of error in Lacaille's catalogue, Mr. Maclear states that he has assumed, pretty freely, the likelihood of certain errors of reading off both the clock and instrument, which are of usual occurrence. This appears to him a more allowable process than to assign large proper motions. Probably, when his task is completed, the merely typographical errors of the *Cœlum Australe* may be got rid of by reference to the manuscripts.

If the zeal of the Cape astronomer and his assistants is supported by health and an adequate supply of labour, we may expect to have a Southern Catalogue, before long, on which we may rely.